BERKELEY'S LOGIC OF MATHEMATICS.*

THAT Berkeley was keenly interested in mathematics is well known. In the *Commonplace Book* a great deal of attention is paid to mathematical questions; it is noticeable, indeed, that in its pages Berkeley refers to mathematicians far more frequently than to philosophers. The extent of his interest in mathematics is indicated also by a group of early writings, *Arithmetica absque Algebra aut Euclide demonstrata,* and *Miscellanea Mathematica* which includes papers "de Radicibus Surdis," "de Cono Aequilatero et Cylindro eidem Sphaerae circumscriptis," "de Ludo Algebraico," and "Paraenetica quaedam ad studium matheseos praesertim Algebrae." Both of these tracts were written in 1705 and first published in 1707. Belonging to the same period is the essay "Of Infinites," which is in part concerned with the infinitesimal calculus. Berkeley deals with mathematical questions also in *The Principles* (1710) and in *De Motu* (1721), and his criti-

* The following article contains, in its treatment of Berkeley's early work which was not published for generations after it was written, a new and important contribution to the history of mathematics. It will also be of interest to our readers to know that editions of the books by Barrow and Wallis mentioned in this article are in preparation. They are edited by Mr. J. M. Child and will appear in the "Open Court Classics of Science and Philosophy." Further, in the same series a small volume by Prof. Florian Cajori on the history of fluxional concepts from the time of Newton is also in preparation. It will contain a detailed account of the *Analyst* controversy. Finally it is to be noticed that Berkeley's doctrine of "compensation of errors" in the calculus was later advocated by the eminent mathematicians Lagrange and Lazare Carnot.—Proofs of this article did not reach the author who was absent on military service.—ED.

cisms of the logical basis of the infinitesimal calculus in *The Analyst* (1734) and *A Defence of Free-Thinking in Mathematics* (1735) are of considerable importance in the history of mathematics.

In this paper I propose to consider the mathematical views stated in Berkeley's *Commonplace Book* and *Analyst*. In both cases he is concerned mainly with the logical basis of mathematics.

Berkeley very clearly perceived that his "new principle" involved difficulties with regard to the nature of mathematics. The "new principle" implied that lines consist of a finite number of points, that surfaces consist of a finite number of lines, and that solids consist of a finite number of surfaces. Thus ultimately all geometrical figures are composed of complexes of points, which are regarded by Berkeley as ultimate individualities. These indivisibles are *minima sensibilia,* the minutest possible objects of sense. It is impossible that the *minimum sensibile* should be divisible, because in that case we should have something of which our senses could not make us aware, and that, Berkeley believes, is simply a contradiction.[1]

Sensation, then, is the test of all geometrical relations. Thus equality depends simply on our inability to distinguish in sense-perception. "I can mean nothing by equal lines but lines which it is indifferent whether of them I take, lines in which I observe by my senses no difference."[2] He explicitly considers the claims of imagination and pure intellect to judge of geometrical relations, and summarily rejects their pretensions. Imagination, he points out, is based on sensation, and has no other authority than that of the senses. It has no means of judging but what it derives from the senses, and, as it is removed by one stage from immediate sense-perception, and has its knowledge

[1] Berkeley's *Works,* Oxford, 1901, Vol. I, p. 86.

[2] *Ibid.,* I, 22.

only at second-hand, it is in fact not so well fitted as sensation to judge and discriminate. Pure intellect, Berkeley continues, has no jurisdiction in mathematics, for it is concerned only with the operations of the mind, and "lines and triangles are not operations of the mind."[3]

Now this view of the nature of geometry is the direct consequence of Berkeley's early metaphysical doctrine, but it is interesting to note that it also connected itself in his mind with the method of indivisibles maintained by the Italian mathematician Cavalieri. "All might be demonstrated," he says, "by a new method of indivisibles, easier perhaps and juster than that of Cavalerius."[4] What precisely Cavalieri meant by his conception of indivisibles is open to doubt, but it is certain that Berkeley's sympathy would be elicited by his demonstration that quantities are composed of indivisible units, a line being made up of points, a surface of lines, and a volume of surfaces. It is possible, though he is very obscure, that he regarded areas as composed of exceedingly small indivisible atoms of area. Berkeley's conception is very similar to this; but whereas Cavalieri maintained that the number of points in a line is infinite, Berkeley was convinced that no line or surface can contain more than a finite number of points, points for him being *minima sensibilia*. This, then, is Berkeley's "new method of indivisibles."

It will follow that geometry must be conceived to be an applied science. The only pure science will be algebra, for it alone deals with signs in abstraction from concrete things. Geometry may be regarded as an application of arithmetic and algebra to points, i. e., the *minima sensibilia* which constitute the whole of concrete reality, Berkeley admits that it is difficult for us "to imagine a minimum,"[5] but the reason is simply that we have not been accustomed to take note of it separately. In reading we

[3] *Ibid.*, I, 22; cf. I, 14. [4] *Ibid.*, I, 87. [5] *Ibid.*, I, 85.

do not usually notice explicitly each particular letter; but the words and pages *can* be analyzed down to these minimal letters. Similarly, though we are not explicitly aware of the *minima sensibilia,* they do exist separately, and may be analyzed as indivisibles in the complex sense-datum presented to us in perception. Geometry, then, is an applied science dealing with finite magnitudes composed of indivisible *minima sensibilia.*

If this conception of geometry be adopted, it immediately follows, as Berkeley very clearly perceived, that most of the traditional Euclidean geometry must be rejected. (1) In the first place, on the new theory, not all lines are capable of bisection.[6] Only those lines which consist of an even number of points can be bisected. If the number of points composing the line be odd, then (supposing bisection to be possible) the line of bisection would have to pass through the central point. But the point is *ex hypothesi* indivisible; hence the line does not admit of bisection. (2) Again, the mathematical doctrine of the incommensurability of the side and diagonal of the square must be rejected.[3] For since both the side and the diagonal of the square consist of a finite number of points, the relation between these lines will always be capable of exact numerical expression. Berkeley even makes the general statement, "I say there are no incommensurables, no surds."[8] (3) It follows that one square can never be double another, for that is possible only on the assumption of incommensurables. And it also follows that the Pythagorean theorem (Euclid, I, 47) is false.[9] (4) Further, it is no longer possible to maintain that a mean proportional may be found between any two given lines. A mean pro-

[6] *Ibid.,* I, 79, 80.
[7] *Ibid.,* I, 60, 78, 79.
[8] *Ibid.,* I, 14.
[9] *Ibid.,* I, 19.

portional will be possible, on Berkeley's theory, only in the special case where the numbers of the points contained in the two lines will, if multiplied together, produce a square number.[10] (5) Finally, the important work that had recently been done on the problem of squaring the circle is, in Berkeley's view, quite useless. Any visible or tangible circle, i. e., any actually constructed circle, may be squared approximately; and it is therefore time thrown away to invent general methods for the quadrature of all circles.

That his new doctrine necessitated such a clean sweep of important mathematical propositions, most of which had been accepted for hundreds of years, might well have given pause to an even more confident man than Berkeley; for (to take only one instance) apart from its startling theoretical aspects, serious practical difficulties would arise if some lines should prove incapable of bisection. Berkeley therefore suggests that for practical purposes small errors may be neglected. Though we cannot bisect a line consisting of 5 points, we can divide it into two parts, one containing 3 points, the other 2; and, as the *minimum sensibile* is so minute, it makes no practical difference that the lines are only approximately equal. Berkeley was influenced to make this suggestion by the method of neglecting differences practised in the calculus.[11] If differentials, which are admitted to be something, are overlooked under certain circumstances in the calculus, are we not justified in the new geometry, Berkeley asks, in neglecting everything less than the *minimum sensibile*?[12] The resulting errors will be so slight that the usefulness of geometry,

[10] *Ibid.*, I, 14.

[11] *Ibid.*, I, 85.

[12] It might seem that in our approximate bisection of the line we have neglected a whole *minimum sensibile*. But from the point of view of the error involved in each of the resulting parts we are not guilty of that. Each of the parts ought to contain 2½ points. Now each of the lines obtained by the approximate method differs from this by only ½ a point. Hence the error to be neglected in each case is less than a *minimum sensibile*. And this is the condition laid down by Berkeley.

which it must be remembered is a practical science, will not seriously be impaired.[13]

It is of peculiar interest to notice that Berkeley was influenced to neglect small errors, and to justify his procedure, by the example of the differential calculus. For in the *Analyst,* written nearly thirty years later, he vigorously attacked the method of ignoring small errors in the calculus. What a triumph for his opponents in the *Analyst* controversy if they could have seen the *Commonplace Book*!

But though Berkeley made use of the illegitimate method suggested by the calculus, his attitude to the calculus itself was from the first exceedingly critical. And his motive for criticism is not far to seek. If the calculus were sound, then his conception of geometry could not be maintained. For the calculus, whether in the form of Newton's theory of fluxions or Leibniz's method of differentials, rested, Berkeley believed, on the assumption of the existence of infinitely small quantities. Now if these infinitesimals were admitted to exist the significance of his *minima sensibilia* would disappear, and indeed the foundations of his philosophy as a whole would be seriously shaken. For if quantities could be proved to exist which were neither sensible nor imaginable he would need to revise his theory of knowledge altogether. He therefore had every reason to look with critical eyes on the conception of infinitely small quantities.

In the *Commonplace Book* he says nothing of importance with regard to the use to which infinitesimals are put in the calculus. Yet even then he was certainly acquainted with a good deal of the work that had been done on fluxions and differentials. His notes contain references, on matters connected with infinitesimals, not only to Newton and Leibniz but also to Barrow, in whose *Lectiones*

[13] *Ibid.,* I, 78.

opticae et geometricae (1669) was given the chief impulse to Newton's theory of fluxions; to Wallis (1616-1703), whose *Arithmetica infinitorum* (1656) paved the way for the invention of the calculus; to Keill (1671-1721), who, in addition to his *Introductio ad veram physicam* (1702), had written of fluxions in the *Philosophical Transactions* of the Royal Society, and took a prominent part in the famous "Priority controversy" in which he accused Leibniz of having derived the fundamental ideas of the calculus from Newton; to Halley (1656-1742) who in addition to his works on astronomy and magnetism wrote on fluxions in the *Philosophical Transactions*; to Cheyne (1671-1743), whose *Fluxionum methodus inversa* (1703) and *Philosophical Principles of Natural Religion* (1705) gained him admission to the Royal Society; to Joseph Raphson, whose *De Spatio reali seu ente infinito* (1697) contained a definition of the infinitely small, and who was afterwards to write a *History of Fluxions*; and also to two more elementary writers, Hayes (1678-1760) who published in 1704 his *Treatise of Fluxions,* and John Harris whose *New Short Treatise of Algebra. . . . Together with a Specimen of the Nature and Algorithm of Fluxions* (1702) was the first elementary book on fluxions to be published in England. And that he had not confined his reading to English works is proved by his reference to the *Analyse des Infiniments Petits,* and to the controversy between Leibniz and Bernhard Nieuwentijt, a Dutch physician and physicist, which took place in 1694-5 in the pages of the Leipsic *Acta Eruditorum.*[14]

It is clear, then, that even when the *Commonplace Book* was written Berkeley was acquainted with much of the work that had been done in the calculus. But at that time he was not in possession of the arguments which he

[14] The last-mentioned references occur, not in the *Commonplace Book,* but in the essay "Of Infinites" (*Works,* III, 411).

afterwards advanced against it in the *Analyst*.[15] In the *Commonplace Book* he does not venture any criticism in detail of the use of infinitesimals *in the calculus*.[16] What he is concerned to do there is to prove that infinitesimals have no real existence at all.

His line of argument is indicated twice over, and is based on his own metaphysic. For the purpose of his proof he posits two axioms: (I) "No word to be used without an idea," and (II) "No reasoning about things whereof we have no idea." Now we have no idea, Berkeley says, of an infinitesimal. By this he means, if his terminology be translated, that infinitesimals cannot be either objects of sense-perception or objects of representation in imagination. Hence, as we have no idea of an infinitesimal, it is simply a word. Further, according to axiom I, it is a word which means nothing; and, according to axiom II, we have no right to use it in our calculations.

We have now considered in outline Berkeley's attitude, as revealed in the *Commonplace Book,* to contemporary mathematical problems. His willingness to throw overboard the solid achievements of the established geometry simply because they did not accord with an *aperçu* of his own does not encourage us to rate his mathematical ability very highly. Or perhaps it would be truer to say that when he wrote the *Commonplace Book* he had not had time to steady his outlook upon science and the world; and allowance may fairly be made for his youthful dreams of a new idea which was destined to revolutionize the sciences, when we remember that it was only about seventy-three years since Galileo expounded the Copernican theory and thus changed entirely the orientation of astronomy and indeed of science as a whole. Another Copernican change,

[15] Some of his remarks show that he was at that time, far from understanding its principles and methods (Cf. *Commonplace Book,* I, 84f).

[16] But there is some criticism of the calculus itself in the essay "Of Infinites" (*Works,* III, 411). And cf. *Commonplace Book,* I, 83-86.

he believed, was not impossible; and in any case he was inclined to think that the wonderful mathematical renaissance of the previous few decades had, among all its triumphs, grown not a few excrescences which it would do no harm but much good to pare off. What he really wished to do was to examine the logical basis of mathematics. He did not advance very far in the *Commonplace Book,* but it was part of what he attempted, and with greater success, in the *Analyst.* To the argument of the *Analyst* we now turn.

The *Analyst* was published in 1734. It is a curious work, and though its purpose is ultimately theological rather than mathematical, it gave rise to a mathematical controversy which lasted for several years and produced more than thirty controversial pamphlets and articles. We have no concern with the theological argument of the *Analyst,* but before passing to consider its mathematical importance, it may be well to mention that the essay is primarily intended as a defense of Christianity, and that Berkeley, acting on the principle that the best defense is in attack, criticizes the foundations of mathematics on the same lines as those on which Christianity had been opposed by "mathematical infidels." In reply to the criticism that the dogmas of Christianity are mysterious and incomprehensible, Berkeley maintains that mathematics, universally admitted to be the most demonstrable department of human knowledge, is, in that regard, in precisely the same position as Christianity. For it also makes use of mysterious and incomprehensible conceptions, e. g., fluxions and infinitesimals. If mathematicians accept mystery and incomprehensibility in mathematics they have no right to object to it in Christianity. This is the kernel of Berkeley's argument.

Berkeley is often regarded, but quite unjustly, as an enemy of the infinitesimal calculus. In reality he had no objection in the world to the calculus as such. What he

did was to submit its logical basis to a searching examination. He criticized the conception of infinitely small quantities, which were at that time vaguely conceived as neither zero nor finite, but somehow in an intermediate state. They were said to be "nascent" and "evanescent" quantities, not quite nothing and not quite anything. It was against this "vague, mysterious and incomprehensible notion" that all Berkeley's attacks were directed; and as soon as it was clearly pointed out by one of the parties to the controversy, Benjamin Robins,[17] that the calculus did not necessarily involve this conception of infinitely small quantities, but might be demonstrated by the methods of limits, the controversy was abandoned by Berkeley. He had replied to his other critics, such as Jurin of Cambridge ("Philalethes Cantabrigiensis") and Walton of Dublin, because these mathematicians persisted in trying to defend the conception of infinitely small quantities. But as soon as it became clear, and Robins was the first to make it so, that that conception was not essential to the calculus, the controversy lost interest for Berkeley. For the method of limits, as he seems to have realized, is not incomprehensible; and therefore an attack on it would not have enabled him to use his *tu quoque* argument, and would thus no longer serve his purpose, which, it must be remembered, was primarily theological.[18]

[17] Robins's contributions to the controversy were contained in his *Discourse concerning the Nature and Certainty of Sir Isaac Newton's Methods of Fluxions, and of Prime and Ultimate Ratios* (1735), and in a series of articles in the *Republic of Letters* in 1736 and in the *Works of the Learned* in 1737.

[18] The course of the *Analyst* controversy, so far as Berkeley was concerned, was as follows. In 1734 the *Analyst* appeared. It was almost immediately attacked by Jurin in an anonymous tract entitled *Geometry no Friend to Infidelity; or a Defence of Sir Isaac Newton and the British Mathematicians.* To this Berkeley replied in *A Defence of Free-Thinking in Mathematics,* published in March, 1735. To this reply Jurin wrote a rejoinder which was published in July of the same year. Berkeley took no notice of it.
Berkeley had another critic. This was Walton of Dublin, who produced in 1735 a *Vindication of Sir Isaac Newton's Fluxions.* It was replied to in an appendix to the second edition of *A Defence of Free-Thinking in Mathematics.* Walton replied, and Berkeley then published his *Reasons for not replying to*

But though his motive in writing the *Analyst* was theological, the chief importance of the book, as we must now try to show, is mathematical. It is, indeed, an able treatise on the logic of mathematics. Berkeley saw that the brilliance of the rapidly accumulating results attained by means of the calculus had tended to put into the background the question of its logical basis and the validity of the methods employed by it. And he did good service to mathematics by the publication of the *Analyst,* for he forced upon mathematicians the investigation of the logical basis of the new mathematics. "I have no controversy," he says, "about your conclusions, but only about your logic and method.... I beg leave to repeat and insist that I consider the geometrical analyst as a logician, i. e., so far forth as he reasons and argues; and his mathematical conclusions, not in themselves, but in their premises, not as true or false, useful or insignificant, but as derived from such principles and by such inferences."[19] As a direct result of this investigation originated by Berkeley two highly important principles were firmly established, (1) that the calculus must be grounded on the method of limits, and (2) that the then current conception of infinitely small quantities must be abandoned.

These points will become clear if we examine Berkeley's criticism of Newton's theory of fluxions. In our investigation there are three main questions which we must ask. (1) Is Berkeley's criticism of Newton valid? (2) Is his criticism of current conceptions of infinitesimals

Mr. Walton's Full Answer. All this took place in 1735. Walton issued a rejoinder, but Berkeley took no further part in the controversy.

It is noticeable that Berkeley participated in the controversy vigorously until Robins's book appeared. After that he said not a word. The reason is, as we have suggested, that Robins showed that infinitesimals are not essential to the calculus. Berkeley must have been convinced by his arguments, and therefore realized that it was no longer possible, from his point of view, to take part in the controversy.

[19] *The Analyst,* § 20.

sound? (3) Did he really expose any fallacies in the calculus?

1. First, then, we must consider whether Berkeley is successful in his criticism of Newton. There is one special point in Newton's theory which must be examined with some care, for upon it depends the applicability of Berkeley's criticism. The question is this. Did Newton really use the conception of infinitely small quantity (in which case he would be exposed to the full force of Berkeley's arguments), or was his method really that of limiting ratios (in which case Berkeley's criticisms would be directed, so far as Newton is concerned, against a man of straw)?

It is often held that Newton never used the conception of infinitely small quantity, but it was conclusively established by De Morgan that this conception does appear in some of his works. De Morgan maintains that until the year 1704 when his *Opticks* was published Newton did believe in infinitely small quantities. "In Newton's earliest papers," writes De Morgan, "the velocities are only differential coefficients: when A changes from x to $x + o$, B changes from y to $y + oq/p$, the velocities being p and q. Those terms in which o remains are 'infinitely less' than those in which it is not, and are therefore 'blotted out.' And 'those terms also vanish in which o still remains, because they are infinitely little.'"[20] Again, in the first edition of the *Principia,* published in 1687, fluxions are founded on infinitesimals, moments being regarded as infinitely small quantities. De Morgan confirms this by relevant quotations from Newton's *Method of Fluxions,* written in the period 1671-1676) and his *Quadratura Curvarum,* which was originally written about the same time, though

[20] "On the Early History of Infinitesimals in England," *Philosophical Magazine,* 1852, IV, 322-3.

it was not published till later. So far, Newton certainly made use of the conception of infinitely small quantity.

But in 1704 the *Quadratura Curvarum* was issued in an appendix to the *Opticks*. It contained a new preface with some important statements regarding infinitesimals. "I here consider mathematical quantities," Newton says, "not as consisting of minimal parts, but as described by continuous motion."[21] "I was anxious to show that in the method of fluxions there is no need to introduce into geometry figures infinitely small."[22] Now Berkeley was well aware that the conception of infinitesimals had been thus disclaimed by Newton. In the early essay "Of Infinites" he says, "Sir Isaac Newton, in a late treatise,[23] informs us his method of fluxions can be made out *a priori* without the supposition of quantities infinitely small."[24]

But in 1713, when the second edition of the *Principia* was published, Newton again admitted, though very obscurely, infinitely small quantities.[25] From all this we may conclude that while Newton did not give exclusive adhesion to the method of infinitesimals, yet the conception of infinitely small quantity does occur in his writings prior to 1704, and though it was renounced in that year it reappears in the second edition of the *Principia* in 1713. It therefore follows that Berkeley's criticism is pertinent. Newton, we have decided, did maintain the existence of

21 "Quantitates mathematicas non ut ex partibus quam minimis constantes, sed ut motu continuo descriptas hic considero."

22 "Volui astendere quod in methodo fluxionum non opus sit figuras infinite parvas in geometriam introducere."

23 This refers to the *Quadratura Curvarum*. Berkeley's "Of Infinites" was written about 1706-7.

24 Berkeley's *Works*, III, 412.

25 This point has been regarded as open to doubt. It depends on Newton's definition of moment. The definition is stated somewhat differently, but very obscurely in both cases, in the first and second editions of the *Principia*, in Book II, lemma II. But Edleston cites a letter from Newton to Keill written in May, 1714, in which he says explicitly, "Moments are infinitely little parts" (J. Edleston, *Correspondence of Sir Isaac Newton and Professor Cotes*, p. 176). This seems to be conclusive evidence that Newton still clung to infinitesimals.

infinitely small quantities, and it is against these that Berkeley argues.

Berkeley points out a serious inconsistency in Newton's conception. He shows that at one time Newton admits that infinitely small moments may under certain circumstances be altogether omitted in calculation. Against this admission he arrays Newton's declaration that in mathematical operations even the smallest errors must not be overlooked. Now the former of these statements is made by Newton in the *Principia* and the latter in the *Quadratura Curvarum.* The two are obviously inconsistent. Berkeley's critics in the controversy tried to defend Newton in various ways, but neither of them dared to admit, even if they perceived it, that the inconsistency was due to a change in Newton's system. In the *Principia*, holding a conception of infinitesimals, he is forced, precisely as the continental exponents of the differential calculus were forced, to admit that infinitely small quantities are negligible in calculation in comparison with those of finite magnitude. On the other hand, in the *Quadratura Curvarum,* having renounced infinitesimals, he is free to assert that even the smallest errors cannot be permitted. Benjamin Robins was the first of Newton's defenders to see clearly that the systems are different, and that if Newton's position is to be seriously defended it will be necessary to admit frankly the change of system, and to maintain that for Newton the really fundamental method is the method of limits.[26]

[26] Berkeley has been accused of bad faith in advancing this criticism. He must have seen, it is argued, that the Newton of the *Principia* was in a different position from the Newton of the *Quadratura Curvarum,* and therefore he was not justified in arraying the statements of these two periods against one another as evidence of present inconsistency (cf. A. De Morgan, *op. cit.,* p. 329). But such an argument overlooks two or three very material facts. The first is that Newton himself nowhere explicitly admits a change of system; in fact he seems anxious to conceal that such a change had taken place. Further, with the exception of Robins, Newton's followers were far from clear whether or not a change had taken place; and, in any case, as we have seen, Newton seems to have returned to the conception of infinitely small

This is what Robins did, and it has come to be realized that the conception of limits forms the true logical basis of the calculus. Berkeley's general criticism of Newton is perfectly valid, and it was largely owing to his objections that the difference between the two methods came to be fully appreciated, and that eventually a method of limits akin to that of Newton was established as the foundation of the calculus.

But in two respects Berkeley was unfair to Newton.

a. He never lets his reader know that Newton used the method of limits at all, and always speaks as if Newton had always held that the method of infinitesimals was essential to his theory of the calculus. Now the truth is, as Robins pointed out, that everything of fundamental importance in Newton's work is perfectly consistent with the method of limits.

b. He gives Newton no credit for his doctrine of continuity. Newton's infinitesimals are, after all, never so self-contradictory as those of Leibniz or even of his own followers. His infinitely small quantities are not, like Leibniz's differentials, discrete particulars. The Leibnizians hold that the "difference" of a line is an infinitely little line, the "difference" of a plane an infinitely little plane, and so on. And Newton's own followers used the conception of infinity in an equally rash way. Thus De Moivre regards the fluxion of an area as an infinitely small rectangle; and Halley, to whom Berkeley refers in the *Commonplace Book,* speaks of infinitely small *ratiunculae* and *differentiolae* in much the same way as the Leibnizians. Hayes, again, another follower of Newton to whom Ber-

quantity in 1713. Now, the *Analyst* was not published till 1734, and at that distance of time Berkeley may quite well have regarded Newton's renunciation of infinitesimals in 1704 as a temporary aberration. In that case he would be perfectly justified in his criticism.

[27] For an appreciation of Benjamin Robins, see Prof. G. A. Gibson's review of Cantor's *Geschichte der Mathematik* in *Proc. Edin. Math. Soc.,* 1899, pp. 20ff.

keley also refers, maintains the conception of infinitely small quantity with much frankness. "Magnitude," he says, "is divisible *in infinitum*. Now those infinitely little parts, being extended, are again infinitely divisible; and those infinitely little parts of an infinitely little part of a given quantity are by geometers called *infinitesimae infinitesimarum* or *fluxions of fluxions*."[28] But Newton himself does not speak in that way. He never forgets that his whole system is based on the continuity of motion. Lines are generated by the motion of points, planes by the motion of lines, and solids by the motion of planes. Fluxions are strictly the velocities of the generating motions. And the continuity of motion, generating lines, surfaces, and the like with varying velocities involves the conception of prime and ultimate ratios. To this aspect of Newton's theory Berkeley seems to be blind.

2. Having considered the respects in which Berkeley's criticism of Newton is valid, we may now proceed to ask whether his criticism of infinitesimals in general will bear examination. The general criticism of infinitesimals consists of two arguments, only one of which seems to be sound.

a. Berkeley argues—to take first the contention that seems unsound—that infinitesimals are impossible because imperceptible. An infinitely small quantity cannot be the object either of sense-perception or of imagination, and in accordance with the formula *esse est percipi* it can therefore have no existence. "As our sense is strained and puzzled with the perception of objects extremely minute, even so the Imagination, which faculty derives from Sense, is very much strained and puzzled to frame clear ideas of the least particles of time, or the least increments generated therein; and much more so to comprehend the

[28] *A Treatise of Fluxions*, 1704. Quoted by A. De Morgan in *Essays on the Life and Work of Newton*, edited by P. E. B. Jourdain, Chicago and London, 1914, p. 91.

moments, or those increments of the flowing quantities *in statu nascenti,* in their very first origin or beginning to exist, before they become finite particles.[29]

Now, this argument is simply at the level of picture-thinking. It does not follow that what we are unable to perceive in sense-perception or to represent in imagination is non-existent. At one time Berkeley's new principle would have necessitated this argument, but when the *Analyst* was written he had outgrown the cruder form of his early theory, and in his doctrine of notions he admitted that we can have knowledge which comes neither through sense nor imagination. He was thus prepared to allow that we might have real knowledge not sensuous in its origin. His retention of the argument here is a sign that he was not completely emancipated from his early sensationalism.

b. Berkeley's second general argument against infinitesimals is perfectly sound. He points out that the conception of the infinitely small, whether in the form in which it appears in Newton and his followers or as maintained by Leibniz, is impossible. It is impossible because it is self-contradictory. Whether we regard infinitesimals with Leibniz as differences, i. e., as infinitely small increments or decrements, or with Newton as fluxions, i. e., velocities of nascent or evanescent increments, they involve in their nature an ultimate contradiction. On the one hand, an infinitesimal seems to be something, for otherwise it would not be used in mathematics; but on the other it seems to be nothing, for mathematicians say it may be neglected in calculations without affecting the accuracy of their results. Sometimes it is called a nascent quantity, i. e., one which has left being nothing, but has not yet quite become anything; at other times it is called evanescent, i. e., a quantity which is still something but is tending to be almost

[29] *The Analyst,* § 4.

(though not quite) nothing. This conception, Berkeley insists, is ultimately incomprehensible and contradictory. His argument here is, of course, perfectly sound. Infinitesimals, conceived in this vague and loose way, have now, very largely owing to the process of criticism initiated by Berkeley, been entirely extruded from the calculus.

3. The last problem which we set before ourselves is this. Did Berkeley, apart from stimulating the investigation of the logical basis of the calculus, expose any real errors in it? From his arguments in the *Analyst* it would seem that two main errors affect the calculus. Berkeley maintains that (*a*) any attempt to demonstrate the value of a fluxion involves the violation of ultimate logical principles, and (*b*) the maxim that infinitely small errors compensate one another is vicious. A word or two must be said on each of these points.

a. In order to prove the illogicality of the methods of determining the value of fluxions, Berkeley examines in some detail the two independent demonstrations given by Newton. In the *Principia* Newton gives a geometrical proof, in the *Quadratura Curvarum* an algebraic one. In each case, Berkeley seeks to show, a closely analogous error is committed.

Take first Newton's geometrical demonstration. We wish to find the fluxion of the rectangle AB generated by the continuous motion of one side upon the other. Let the moments or momentaneous increments of A and B be a and b respectively.

When the sides of the rectangles are each diminished by half their moments, the rectangle becomes

$$(A - \tfrac{1}{2}a)(B - \tfrac{1}{2}b)$$

$$\text{i. e., } AB - \tfrac{1}{2}aB - \tfrac{1}{2}bA + \tfrac{1}{4}ab.$$

Similarly, when the two sides are increased by half their moments, the rectangle becomes

$$(A + \tfrac{1}{2}a)(B + \tfrac{1}{2}b)$$
$$\text{i.e., } AB + \tfrac{1}{2}aB + \tfrac{1}{2}bA + \tfrac{1}{4}ab.$$

Subtract now the former rectangle from the latter, and the remainder is $aB + bA$. This remainder is the moment of the rectangle generated by the moments a, b of the sides. Such is Newton's proof.

In criticism of it Berkeley maintains that the natural and direct method of obtaining the moment of the rectangle AB, when the moments of its sides are a and b, is to multiply into one another the sides increased respectively by their *whole* moments.[30] The moment of the rectangle is therefore

$$(A + a)(B + b) - AB,$$
$$\text{i. e., } AB + aB + bA + ab - AB,$$
$$\text{i. e., } \quad aB + bA + ab.$$

This, Berkeley says, is the true moment or increment. It differs from that obtained by Newton's proof by the quantity ab. Now, as it was essential for the method of fluxions to eliminate the term ab, Newton and his followers said that it was so infinitely small that it could simply be neglected. But against this defense Berkeley quotes Newton's own words, "In rebus mathematicis errores quam minimi non sunt contemnendi."[31]

Berkeley shows that Newton's algebraic proof also rests on illegitimate assumptions.[32] In this demonstration we are given the uniformly flowing quantity x, and it is required to find the fluxion of x^n.

Suppose that x, in process of constant flux, becomes $x + o$, then x^n becomes $(x + o)^n$. Expanding this by the method of infinite series we get

$$x^n + nox^{n-1} + \tfrac{1}{2}n(n-1)o^2x^{n-2} + \ldots.$$

(i. e., the increment of x^n is $nox^{n-1} + \tfrac{1}{2}n(n-1)o^2x^{n-2} + \ldots$).

[30] *The Analyst*, §§ 9ff.

[31] *Opticks*, Introduction to *Quadratura Curvarum*.

[32] *The Analyst*, §§ 13ff.

It follows that the increments of x and x^n are to each other as o to $nox^{n-1}+\frac{1}{2}n(n-1)o^2x^{n-2}+\ldots\ldots$
or, dividing by the common quantity o,

$$\text{as } 1 \text{ to } nx^{n-1}+\tfrac{1}{2}n(n-1)ox^{n-2}+\ldots.$$

Now, "let the increments vanish," and the last or limiting proportion is $1 : nx^{n-1}$. The ratio of the fluxion of x to that of x^n is as 1 is to nx^{n-1}.

Berkeley points out that this reasoning is illogical. If we say, "Let the increments vanish," we imply that the increments are really nothing, seeing that they are negligible. But we are enabled to arrive at the proportion between the fluxions only by assuming that the increments are something. Berkeley accordingly maintains that it is illogical to reject the increments, and still retain an expression, i. e., the proportion of the fluxions, obtained by means of them. If we let the increments vanish, we must also in consistency let everything derived from the supposition of their existence vanish with them.

This criticism Berkeley supports with a lemma, which he states as follows, "If, with a view to demonstrate any proposition, a certain point is supposed, by virtue of which certain other points are attained; and such supposed point be itself afterward destroyed or rejected by a contrary supposition; in that case, all the other points attained threby, and consequent thereupon, must also be destroyed and rejected, so as from thenceforward to be no more supposed or applied in the demonstration."[33]

b. Berkeley goes on to urge that, even though correct results are attained by the application of the method of fluxions, that does not validate the method as method. That the conclusion of a syllogism is true does not necessarily imply that the process of reasoning is correct. The conclusion may be true, and yet logical errors may have been

[33] *The Analyst,* § 12.

committed in the process of proof. It is possible to reach a true conclusion from false premises by erroneous reasoning. One error compensates the other, and thus, though the conclusion is true, the logic is faulty. Precisely similar is the case of the calculus. True conclusions may be attained by it, and results of great practical value may be achieved, but its method is unsound because it is based upon the illogical principle of the compensation of errors.

These are the main arguments advanced by Berkeley in the *Analyst*. In the controversy which ensued all the points that he raised were traversed and re-traversed, with the result that (1) the vague notion of infinitely small quantity was abandoned, (2) the method of limiting ratios was firmly established, and (3) the principle of the compensation of errors was seen to be inconsistent with the logical foundation of the calculus.

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